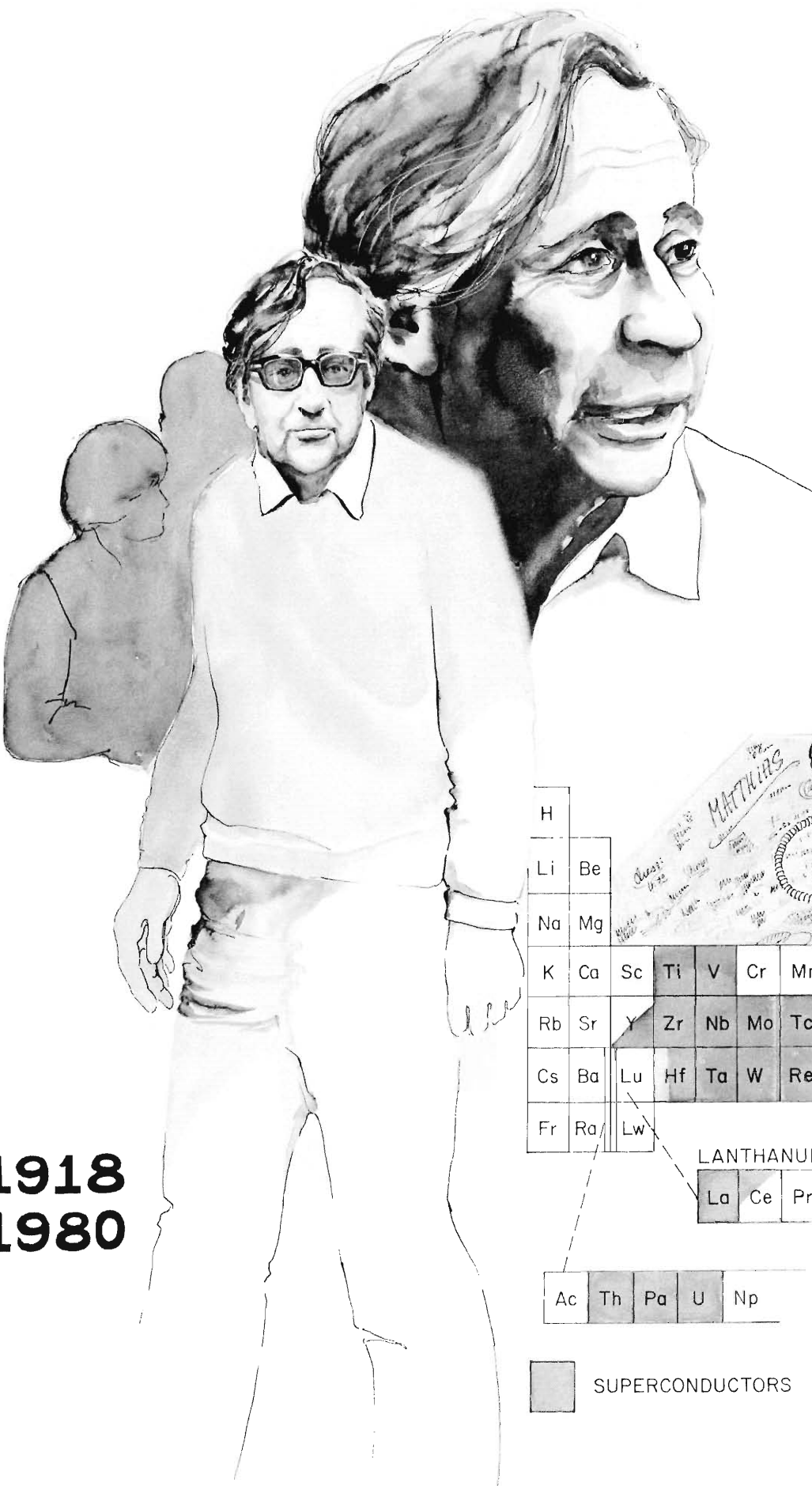


1918
1980



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Li	Be							
Na	Mg							
K	Ca	Sc	Ti	V	Cr	Mn	Fe	Co
Rb	Sr	Y	Zr	Nb	Mo	Tc	Ru	Rh
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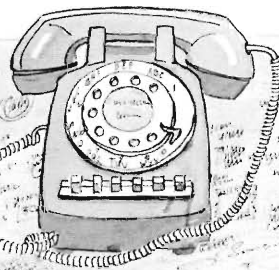
LANTHANUM SERIES

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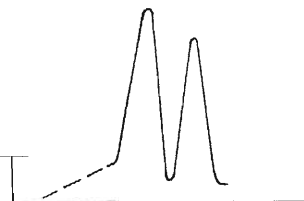
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SUPERCONDUCTORS



Gayle Fulwyler Smith



Bernd Matthias

A Personal Memoir by Paul R. Stein

Some time on the morning of October 27, 1980, Bernd Matthias suffered a fatal heart attack. The news reached relatives and some close friends before mid afternoon; by the next day, hundreds knew. The common reaction among all these people, at least initially, was disbelief. How could a man so vital, so filled with creative energy, be cut down without reason or warning? There were so many plans, so many irons in the fire, so many people depending on him. It was unthinkable, in fact, ridiculous. Of course, acceptance and grief soon followed. But even now, months afterward, there are a few old friends who would say that if they saw Bernd in the street or heard his voice in a crowd, they would not be astonished.

What I have just described is a familiar, much-studied syndrome. The surprising thing is how many people exhibited the symptoms, usually restricted to intimates of the deceased. One is moved to conclude that all these people really felt themselves to be close to Bernd. In my opinion, that conclusion is certainly right.

Early Years

Now for some facts, and, as they say, a little bit more. Actually, in what follows I shall not attempt a strict separation of fact and hearsay except where scientific achievement is involved. Much of Bernd's history is embodied in anecdotes; very often a story, even though merely *ben trovato*,

contains more truth than a register of dates and deeds. But first let me apologize to more formally-minded readers for the practice of referring to my subject, Professor Dr. B. T. Matthias, simply as Bernd. I am not invoking the privilege of long and close friendship; it is rather a matter of what sounds best. Bernd himself had absolutely no use for academic stuffiness, and was unimpressed by titles of any sort. Applied to him, the formal mode of address has a false ring; here I have adopted what I hope is a suitably informal style.

Bernd was born on the 8th of June, in either 1918 or 1919; he himself stuck by the second date, but most others opt for the first. At least there is general agreement that the place was Frankfurt, Germany. There is

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something mildly ambiguous about the record of Bernd's early years, and I shall not be concerned with trying to clear things up. This may be the proper occasion for proposing a new uncertainty principle à la Heisenberg. Although the principle has not yet been clearly formulated, one symbol suggests itself: Δt_B , a time measured in years representing the absolute uncertainty for a subset of significant dates in Bernd's life. Evidently $\Delta t_B \approx 1$.

The milieu Bernd entered was upper middle-class, and there were apparently no financial problems. His father, a successful leather merchant, died when the boy was two. In 1925, Frau Matthias moved her family (Bernd and his younger sister Judith) to nearby Königstein, where she owned a country house with extensive grounds. It must have been a wonderful place indeed, if the tales of idyllic childhood that have come down to us are accurate. All the usual children's games are recorded, the difference being that the playmates were offspring of the aristocracy and the very rich. The boy was bright, unconventional, and undoubtedly spoiled by his mother, who doted on him. Marta Matthias had grand ambitions for her son; she wanted him to be an international figure, preferably in the physical sciences. According to Bernd, as reported by his wife Joan, his mother thought a fitting role would be that of a great astronomer. This suggests that maternal influence was the principal factor in Bernd's choice of career.

Frau Matthias believed in the British system of educating the well-to-do. Accordingly, Bernd attended a series of boarding schools, most of them in Switzerland, which were predominantly international in character. It was probably during this period of secondary education that Bernd laid the foundation for his command of Swiss German, to be perfected during his university years. His remarkable ability to handle—and occasionally manipulate—people, so evident to all who knew him, must also have had its

development in this period. The following fairly well-attested story suggests that this is so. In one of the boarding schools, Bernd had as a classmate the scion of a very rich and aristocratic Italian family. This young man was easily the most disliked student in the entire school. Recognizing the problem and its cure, Bernd offered to make the noble youth one of the most popular boys in the class; for this he would receive, if successful, a certain sum. As the story goes, Bernd succeeded brilliantly, and collected the money. Shortly thereafter the boy's mother came to visit. Bernd feared she would angrily demand return of the fee, which was not precisely in the chicken-feed range. Instead, the lady was overjoyed, and Bernd became a lifelong friend of the family.

The University

In 1936, after a mysterious last fling in Rome, during which he reportedly cashed in on the standing invitation occasioned by the boarding school feat mentioned above, Bernd entered the Eidgenössische Technische Hochschule (E.T.H.) in Zurich. His major was physics. The death of his mother in 1938 seemed to strengthen his resolve to succeed in his chosen field, and he became, by contemporary accounts, a very hard worker. It is said that he was not particularly strong in mathematics. I can believe this, even though in later years he displayed a lively interest in the facts of number theory. Perhaps it was this attitude toward mathematics that kept him away from theoretical physics, despite the presence among his teachers of two very strong theorists, Wolfgang Pauli and Gregor Wentzel. (Later, at the University of Chicago, Wentzel tried, without success, to interest Bernd in field theory.) Bernd became an experimentalist, and his subsequent brilliant career attests the correctness of this choice.

Bernd's thesis advisor was Paul Scherrer, a man in his late forties, whose considerable

zest was the equal of Bernd's own. After receiving his Ph.D. in 1943, Bernd became a research associate, evidently Scherrer's favorite. Despite the age difference, the two were close friends, "even to the extent of exchanging girl friends," as one former schoolmate has remarked. Bernd was admired and envied for his ability to attract pretty girls. "He was an expert in arranging to have girl friends in strategic positions," girls with access to fancy foods (then hard to come by) and girls with political connections. The former provided him and his friends with gourmet fare at his laboratory every morning; the latter enabled him to maintain his work permit despite his lack of Swiss citizenship.

Bernd's thesis involved some properties of ferroelectric crystals; this was to be his main professional interest for the next six years. It soon became apparent that he would not be content merely to refine and extend the work of his predecessors. Bernd was not especially interested in proving or disproving other people's theories. (Perhaps his work in the 1960s on the superconducting isotope effect should be considered an exception.) Instead, he adopted an empirical approach, testing many hundreds (later thousands) of alloys and compounds, regardless of prevailing theories, to find new examples of whatever phenomenon currently interested him. As we know, this approach paid off in the most handsome fashion for superconductors. But even in the early days, before he became interested in superconductivity, he carried out this sort of research on ferroelectrics, most of it in collaboration with John Hulm at Chicago. At E.T.H., from 1944 to 1949, he used much of his time developing his experimental skills and, by no means incidentally, learning to grow pure crystals of barium titanate. As it happened, Bernd's technique became known to crystallographers at the Cavendish Laboratory in Cambridge, and their product was used by Hulm for some important research. Bernd was, in effect, scooped; this

occasioned some acrimonious argument when he and Hulm first met at Chicago in 1949. They soon became close friends, however, and enjoyed many years of fruitful collaboration.

Bernd's empiricism was, I think, a symptom of his intellectual independence. To some, however, he seemed merely brash and impertinent, especially when he challenged in public the views of senior professors. No doubt there was in his character an innate irreverence coupled with a youthful desire to shock. One old friend from the Zurich days recounts a typical incident. Bernd had been invited by some colleagues to join them in a Sunday climb near Schwyz, in the central part of Switzerland. Because he had seemed less than enthusiastic, the others were not surprised when he failed to meet them at the train station. The party of four, clad in all their mountaineering gear, had boarded and the train had started to pull out, when Bernd appeared, casually dressed in shirt, slacks, and sneakers, and carrying a small parcel of pastry for his lunch. So they all climbed together, four in full regalia and one dressed for tennis. Of course Bernd had a cover story, but to me the whole thing sounds like pure panache.

America

By 1947, Bernd was an expert on ferroelectric crystals. This got him an invitation from Arthur von Hippel to come to MIT for a year. The invitation was

event in Bernd's professional life during this period was making the acquaintance of Willie Zachariasen. They were introduced by Ray Pepinsky (who had been one of Willie's early graduate students) during a crystallography meeting at Yale in March 1948. By this time Willie had been recognized as a "world-class" crystallographer, in the forefront of his profession. The two hit it off to an extent that could scarcely have been predicted. It may be that Willie assumed the semipaternal role played previously by Scherrer. At any rate, Bernd and Willie became very close friends, and Willie taught Bernd a great deal about crystal structure, indispensable knowledge for future endeavors. The relationship could not be described as serene. Both men were very outspoken. From 1962 on, I myself witnessed numerous arguments, mostly about technical matters, but occasionally having to do with the relative merits of fellow scientists. Willie was perhaps the sounder of the two, but he was also the more stubborn. So far as I could judge, the really substantial arguments were draws.

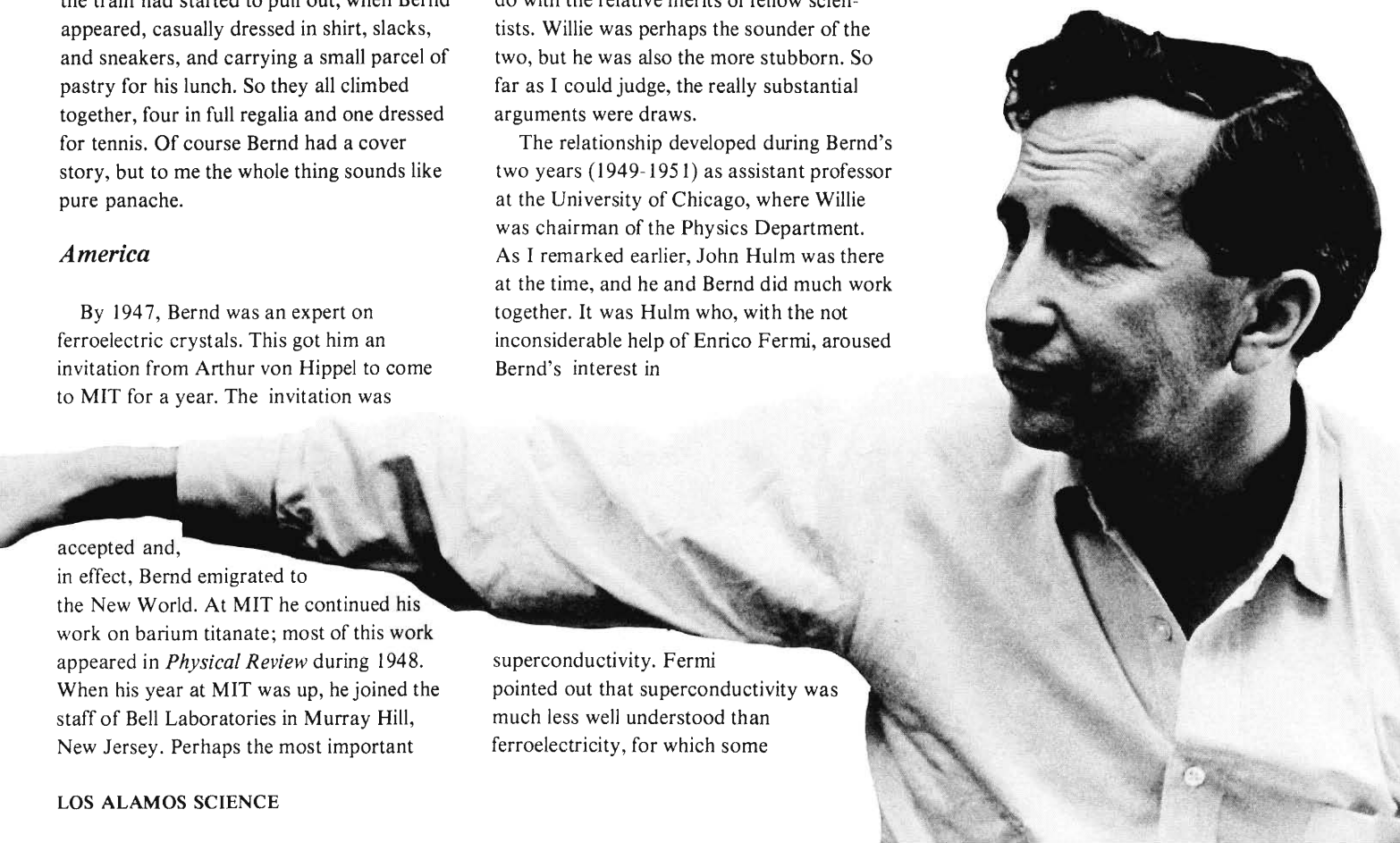
The relationship developed during Bernd's two years (1949-1951) as assistant professor at the University of Chicago, where Willie was chairman of the Physics Department. As I remarked earlier, John Hulm was there at the time, and he and Bernd did much work together. It was Hulm who, with the not inconsiderable help of Enrico Fermi, aroused Bernd's interest in

reasonable theoretical models existed. I don't know what Bernd thought of these models of ferroelectricity. Much later he would claim that ferroelectrics and superconductors are similar in structure: "Superconductivity is a phase which is just on the verge of disappearing; small variations can convert a superconductor into a ferroelectric metal or a semiconductor, or possibly cause the crystal to fall apart entirely."

From a list of Bernd's publications, I would conclude that his serious interest in ferromagnetism also dated from the Chicago period. Twenty-eight years later, in 1978, Bernd had this to say about the relation between ferromagnetism and superconductivity:

accepted and, in effect, Bernd emigrated to the New World. At MIT he continued his work on barium titanate; most of this work appeared in *Physical Review* during 1948. When his year at MIT was up, he joined the staff of Bell Laboratories in Murray Hill, New Jersey. Perhaps the most important

superconductivity. Fermi pointed out that superconductivity was much less well understood than ferroelectricity, for which some



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It is well known that a sufficiently large magnetic field will destroy superconductivity (by interfering with the alignment of the Cooper pairs). One might think this means that magnetic compounds are never superconducting, but that is not so (the example of cerium under pressure has already been mentioned). Superconductivity and magnetism are, after all, kindred phenomena (both ordered states, or condensations) differing, one might say, in sign (superconductivity can be considered an extreme case of diamagnetism). It is therefore not surprising that the presence of magnetism often indicates where one should look for superconductivity. What is surprising is the relative scarcity of magnetism; it is much more interesting to look for magnetic compounds than for superconducting compounds, since the latter can be found nearly everywhere.

According to one source, Bernd thought for many years that magnetism and superconductivity were incompatible. As for the electron-phonon interaction, the principal mechanism in the theory of superconductivity proposed by Bardeen, Cooper, and Schrieffer in 1957, Bernd seems not to have fully accepted it until the West Coast Gordon Conference of 1970.

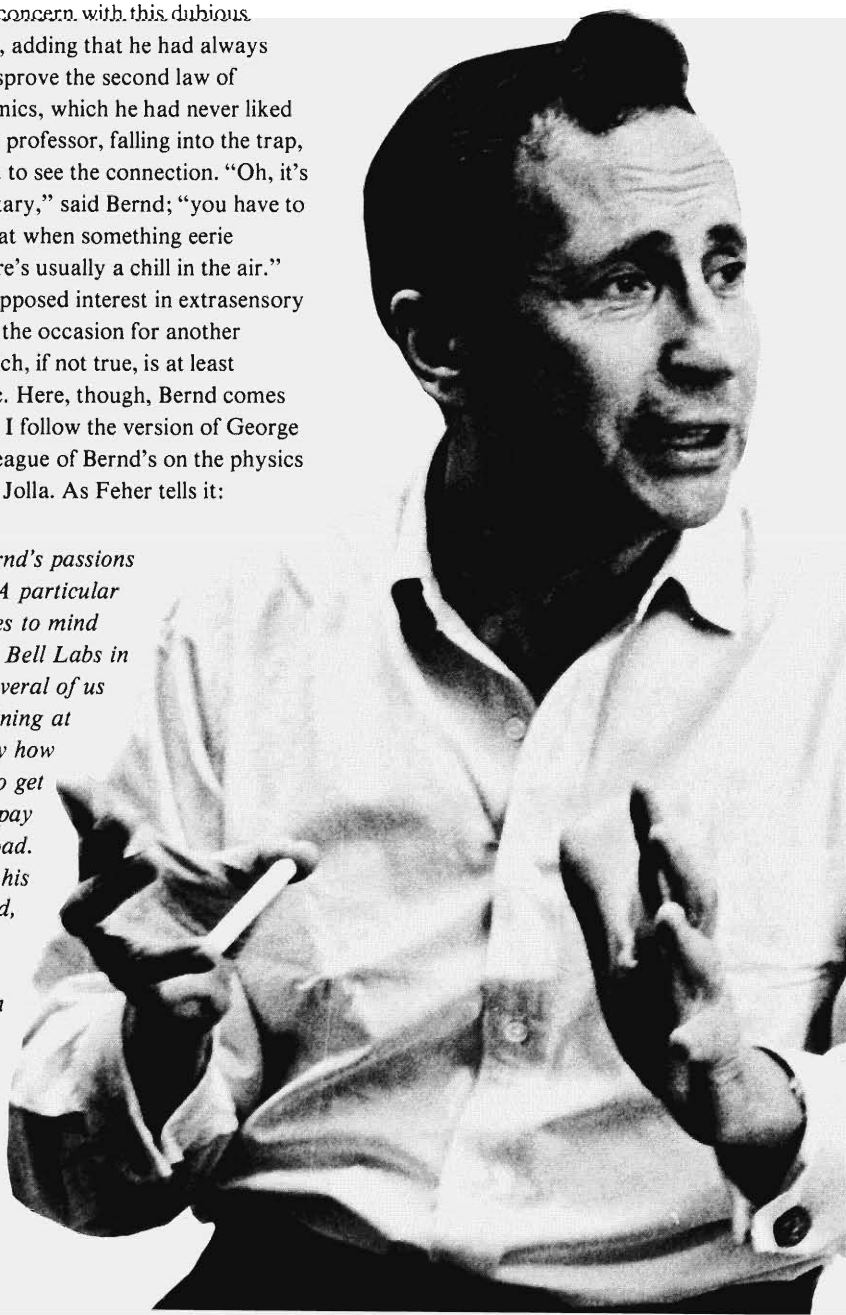
Bernd did not stay long at Chicago, returning to Bell Laboratories after only two years. He was on leave, and it could be that Jim Fisk, then president of Bell, told him to come back or pack up. I myself have always assumed that his leaving Chicago so soon had something to do with the unlikelihood of achieving tenure. Apart from Zachariasen, and to a lesser extent Fermi and Wentzel, Bernd had no influential supporters at either the University or the Metals Institute. Perhaps it was a case of the Bright Young Man ruffling some establishment feathers. Precisely this is said to have happened when Bernd was being interviewed for a job at Princeton. The anecdote, considerably condensed from Bernd's Standard Version, goes as follows. Eugene Wigner arranged the

interview and, just before it took place, pleaded with Bernd to be on his best behavior. (That in itself may have been a mistake.) One senior professor, apparently irritated by Bernd's self-possessed manner, proceeded to unsheath the needle. This did not sit well with Bernd. Things came to a head when the professor, with some sarcasm, asked Bernd about his interest in extrasensory perception. Bernd quickly admitted his concern with this dubious phenomenon, adding that he had always wanted to disprove the second law of thermodynamics, which he had never liked anyway. The professor, falling into the trap, said he failed to see the connection. "Oh, it's quite elementary," said Bernd; "you have to remember that when something eerie happens, there's usually a chill in the air."

Bernd's supposed interest in extrasensory perception is the occasion for another anecdote which, if not true, is at least characteristic. Here, though, Bernd comes out the loser. I follow the version of George Feher, a colleague of Bernd's on the physics faculty at La Jolla. As Feher tells it:

One of Bernd's passions was betting. A particular bet that comes to mind took place at Bell Labs in the fifties. Several of us were complaining at lunch one day how hard it was to get Bell Labs to pay for trips abroad. Bernd shook his head and said, "That's just because you are all ribbon clerks" (one of his favorite expressions). "I'll bet you I can get Bell

to pay me for a trip to Tibet and an extended stay there." I didn't believe he could pull it off, and took him up on it immediately. The next week Bernd arranged a luncheon with Jim Fisk (then president of Bell Labs) and, between the soup and the main course, he said, "Jim, I want to go to Tibet for a few months, and I expect Bell Labs to pay for my trip." When Jim quietly asked why, Bernd replied, "Well, I know of a guru there who



can teach me ESP. Knowing about this should be of enormous importance to the phone company. If ESP became commonplace and everybody could practice it, nobody would need phones, and Bell Labs would go broke." Jim thought for perhaps a second and said, "Bernd, I'm afraid you're behind the times; we already have a department that does nothing but work on the jamming of ESP."

Success

Back at Bell Laboratories, Bernd settled down to some hard work in his new field of superconductivity. "Settled down" may be too strong; at least he had taken the first step toward solid citizenship in 1950 by marrying Joan Trapp—a move that certainly did not hurt his reputation for finding and influencing pretty girls.

The attempt to understand superconductivity led to a search for new superconducting materials. Among Bernd's collaborators in this effort were Ted Geballe, Joe Remeika, the theorist Harry Suhl, and "Bernd's secret weapon" Ernie Corenzwit, who was a coauthor with him on at least 60 papers.

A word about Bernd's publications is in order here. The corpus of 309 papers through 1978 seems at first glance to consist mainly of experimental reports with titles like "Superconductivity in the Y-Rh and Y-Ir Systems" (Matthias, Geballe, Compton, Corenzwit, and Hull). A closer look reveals that about a quarter of the entries are review or state-of-the-art papers; of many of these Bernd is the sole author. In a rapidly changing field there is much demand for such continual updating. The existence of these articles indicates not only that Bernd was fast becoming (and by the early 1960s had become) the authority on practical superconductivity, but also that at fairly regular intervals during the course of his investigations Bernd would attempt to generalize from experience.

As I have remarked, for many years Bernd found it difficult to accept the electron-phonon interaction as the basic mechanism of superconductivity. When I first asked him about it, the BCS theory was roughly five years old. Even in the early days he did not disparage the theory, but merely declared it irrelevant to the discovery of new superconducting materials. His brilliant success in that very endeavor tended to prove his point. Bernd's quarrel with the solid-state theorists grew in intensity through the 1960s, the acrimony reaching a peak in the early 70s. He did not, of course, fight with all of them. Notable exceptions were his friend Harry Suhl, Phil Anderson, Charlie Kittel, Conyers Herring, and probably John Bardeen. Then there was J. H. Van Vleck, whom he admired and with whom he did some important work on europium oxide. But I would have to say that he did not have many friends in the theoretical camp. In the last few years, I thought I detected some signs of mellowing, but now I am not so sure. As late as 1978 he was capable of publishing some very bitter remarks.

The argument between Bernd and the theoretical establishment in solid-state physics had more than a single cause. One was the claim of some theorists to be able to predict superconducting phenomena. In my opinion, Bernd showed convincingly that these claims were empty. Although a considerable showman himself, Bernd would not tolerate charlatanism in science, and that is just how he viewed these attempts at prediction. To be sure, he sometimes carried his criticism to ludicrous extremes—for example, to the point of stating that the mere writing down of a Green's function was a sign of fraudulent intent. If failure to predict was bad, "prediction after the fact" (as he called it) was worse. What Bernd meant by this phrase was the promulgation of theories so full of arbitrary constants that they could be made to fit almost any situation. Experimentalists would discover new phenomena, and the villains of the piece

would adjust their formulae so that in due course they could "explain" the results. Unfortunately, it often happened that the supply of constants was exhausted before everything could be explained—a sure sign of a bankrupt theory.

There was also a philosophical disagreement at the root of the trouble; readers will recognize it as nothing more than the ancient battle between induction and deduction. Bernd's commitment to the empirical method was very strong. He was convinced that constructing a theory and then designing experiments to verify it was the wrong way to study a phenomenon as complicated as superconductivity. The right way was to try everything, in flexible sequence—for example, substituting elements from the same part of the periodic table while maintaining the same crystal structure—to see how the transition temperature T_c actually varied as these changes were made. This "pay-as-you-go" inductive method is not congenial to most theoretical physicists, and it is easy to see a source of conflict here. But Bernd had tangible progress on his side. Not only was he extremely successful in his program, he is also given credit by some for a modest revival of scientific empiricism in this country.

During the eight or nine years at Bell Laboratories, Bernd and his coworkers had many triumphs, but two stand out. One was the discovery in 1954 of the superconductivity of Nb_3Sn at 18 kelvin; the transition temperature was a record high at the time. Although several people were involved—Matthias, Geballe, Geller, and Corenzwit—Bernd seems to have been the driving force behind the work, and he received much acclaim for his efforts.

Niobium-tin has the so-called beta-tungsten crystal structure (a misnomer arising from an early, mistaken identification of a "new" form of tungsten), which is still the most favorable structure known for high transition temperatures in binary

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compounds. Nb_3Sn is a “type II” superconductor with a high critical field. Commercially useful superconducting magnets came into being in 1960 when long wires of Nb_3Sn became available for winding solenoids.

The other outstanding triumph was the formulation of an empirical rule to guide the search for superconductors with high transition temperatures. Over the years, Bernd and his coworkers had observed that, in those parts of the periodic table where superconductors generally occur, there is a correlation between transition temperature and the number of valence electrons per atom. For non-transition-metal superconductors, the peak transition temperature is found at about 5 valence electrons per atom; for transition-metal superconductors, there are peaks at both 5 and 7 e/a. In the case of binary compounds one uses the weighted average for the two components. Of course, this prescription is not infallible; it cannot, for example, distinguish between La_3In and LaIn_3 , (lanthanum and indium have the same number of valence electrons), yet these two compounds have quite different transition temperatures (10.5 and 0.7 kelvin, respectively). Further, it fails completely for ternary compounds. Nevertheless, the “electron counting” prescription, first published in 1955, proved to be one of Bernd’s principal tools in his discovery of over 1000 superconducting materials.

Los Alamos and La Jolla

In the early 1950s Nick Metropolis suggested to Carson Mark, then leader of the Laboratory’s Theoretical Division, that Bernd would be a good man to have on the Division’s consultant list. Nick had come to know Bernd at Chicago and had formed a very favorable opinion of his abilities. Carson was persuaded, and the wheels were set turning. Apparently they turned rather slowly (the clearance check proved hard to

complete), but finally Bernd appeared at Los Alamos. The vexed question of just when this happened is further proof that $\Delta t_B \approx 1$; circumstantial evidence points to 1956, but strong, unshakable memories say 1957.

Bernd’s consultant duties were at best loosely prescribed; based as he was in the eclectic corridors of the Theoretical Division, he was free to look into anything that interested him. Chemistry, Metallurgy, and Cryogenics were where one might have expected to find him, but early on one was more likely to run into him in one of the weapons-design divisions, where he dispensed expertise on piezoelectric switches. It was not until 1962 that a paper on superconductivity appeared bearing his name along with those of Laboratory members. Very early he began to be used in an advisory capacity; he also acted as self-appointed liaison between initially incompatible groups. Ultimately, of course, there was a great deal of superconductivity research done at Los Alamos either under his direction or with the benefit of his advice. After 1970 Bernd had closer contact with the Director’s Office, and in 1971 Harold Agnew appointed him a Fellow of the Laboratory, the first to be so honored.

A branch of the University of California was established in La Jolla in 1960; in 1961 Bernd was appointed Professor of Physics at the new institution (named, for some reason, UCSD rather than UCLJ). Despite the move to California he maintained his connection with both Bell Laboratories and Los Alamos. This meant there were now three laboratories at which Bernd’s superconductivity research could be pursued. Among those who from time to time worked with Bernd at Los Alamos were Al Giorgi, Gene Szklarz, Clayton Olsen, Bernd’s student the late Hunter Hill, and, in later years, Jim Smith and Greg Stewart. Smith and Stewart, along with Giorgi, collaborated with Bernd in a remarkable study of the eutectic structure of yttrium-iridium; this was the last project Bernd was involved with at Los Alamos.

In La Jolla Bernd had his old friend Harry Suhl, the only theorist with whom he published regularly. Between 1965 and 1980 he supervised 22 doctoral theses, and several of his former students remained at La Jolla for extended periods. Among these were Brian Maple, George Webb, Zach Fisk, Angus Lawson (now at Pomona), and the late John Engelhardt.

In these years Bernd developed a coherent view of the nature of superconductivity and the limits on the transition temperature. To mention only one aspect, Bernd came to believe that in binary compounds with the beta-tungsten structure, perfect stoichiometry was essential for attaining high transition temperatures. This is well illustrated by the case of Nb_3Ge . The original niobium-germanium compound had the composition $\text{Nb}_{3.3}\text{Ge}$, and was superconducting at 6 kelvin. It took many years to develop techniques for making a compound with a composition closer to the 3-to-1 ratio, for example, $\text{Nb}_{3.1}\text{Ge}$, with a transition temperature of 18 kelvin. Finally, in 1973, using the “sputter” technique, John Gavaler at Westinghouse Research Laboratories was able to make the compound Nb_3Ge ; it has a transition temperature of 23 kelvin, the highest of all superconducting materials. Of course, Nb_3Ge is quite unstable; just dropping the sample on the ground will lower the transition temperature. It would be technically advantageous to find a superconductor with a transition temperature of 25 kelvin or higher, because then liquid hydrogen rather than liquid helium could be used as the coolant. Such a material has so far eluded discovery. Bernd conjectured that niobium-silicon would be such a compound if it could be made in the exact stoichiometric ratio of 3 to 1, that is, as Nb_3Si , but went on to say, “. . . this does not seem possible at present. The basic reason for this is that the silicon atom is just too small; in the beta-tungsten structure, stoichiometry seems to require atoms with

comparable radii.” I am told that the question is very much alive.

Because of the extremely unstable nature of superconductors with high transition temperatures, Bernd believed that T_c values greater than about 30 kelvin were unattainable with any material, and that 25 kelvin was probably an upper limit for binary compounds with the beta-tungsten structure. Therefore, he was greatly incensed to discover that much government money was being used to support a search for organic superconductors (in the form of thin polymers) that would, it was hoped, be superconducting at room temperature or thereabouts. Bernd was adamant (his favorite word) in his opinion that such things did not and could not exist. Around his laboratory one could hear occasional jokes about “superconducting carrots” and “high T_c celery.” Privately, Bernd did not deny the possibility that some organic crystals might be superconducting, but if such things were found, they would (he said) have very low transition temperatures. When a meeting on organic superconductors was announced, Bernd stated (probably incorrectly) that it was “the first conference ever on a nonexistent subject.” Bernd said on more than one occasion that if the government wanted to waste its money on ridiculous projects, it would do better to fund the development of antigravity paint—because if anyone ever managed to make that, the payoff would be beyond imagining. The existence of some superconducting organic crystals has recently been reported; as Bernd suspected, these materials have low transition temperatures and are of no immediate practical interest.

The Professor

Bernd had been a professor before, but his new position bore no resemblance to the previous one at Chicago. All of a sudden he was on top of the academic heap, with perquisites that effectively freed him from the

more onerous duties. For one thing, his relation to the University curriculum was extremely tenuous; he was never required to teach a regular course, graduate or undergraduate. To be sure, he did conduct some advanced seminars in solid-state physics. I heard him lecture at the first meeting of one such seminar. To me it seemed an unabashed exercise in one-upmanship. I trust the subsequent sessions were more productive. The real thrust of his teaching was directed at his many graduate students. According to the testimony of some of these former students, Bernd was a splendid teacher. His method was to probe and challenge, aiming to develop self-reliance in the pupil. Despite his advocacy of the Socratic method, he did not withhold advice. I have often heard him giving his views to a student in informal discussion. It is reported that much of this teaching occurred in his laboratory after midnight (I was not a witness to that); he would touch base with his students after having spent an hour or so carefully looking through the current literature.

Bernd was quick, and exceptionally good at thinking on his feet, very much in the style of the best theoretical physicists I have known. And like them, he was not invariably sound. This was never more evident than when he and Zachariasen argued. Willie, too, was quick, but his response tended to be more of a lecture than the series of rather aphoristic remarks one usually heard from Bernd. Willie, by the way, came out to La Jolla almost every spring to work his indispensable and irreplaceable magic. He also talked with Bernd’s students (on request), sometimes, it seems, to great effect.

In a short piece published last year, three of Bernd’s former students (Zachary Fisk, Brian Maple, and George Webb) gave a moving description of what it was like to study under him. Among other things, they remarked on the great social rapport he maintained with his students: “Friendship and science became intertwined. Interaction

spilled over into parties, weekend trips, and countless sessions at the famous El Sombrero cantina in the village of La Jolla.” (One gratuitous comment: for anyone with a musical ear, the now defunct Sombrero was a dismal swamp.)

One gift that heightened the impact of Bernd’s personality on his students and nonstudents alike was his eloquence. Bernd spoke a fluent, imaginative, and slightly inaccurate English. I think his unconventional phraseology was actually an advantage for him; it tended to fix metaphors in the listener’s memory. In retrospect, I regret having persuaded him to suppress some of the more unusual locutions. One I unfortunately helped get rid of was “forth and back” (from the German *hin und her*). Bernd put up a weak logical argument in its favor, but finally bowed to the overwhelming evidence that “back and forth” was the accepted form. His accent was not bad and rarely caused problems. I do, however, remember one occasion when it almost got him into trouble. The Matthiases, Zachariasens, and Steins were driving through the border checkpoint at Tijuana, on our way to Ensenada. Two burly Mexican cops spotted Bernd as a foreigner, and started to give him a hard time. Bernd very wisely adopted a docile, conciliatory attitude, and eventually we got through. As we drove away, I thought to ask “Willie, how about you? Why didn’t they pick on you too?” “Ah,” he said, in his unmistakable Norwegian accent, “when they heard me talk, no doubt they thought I was from New England.”

In writing, Bernd’s exotic style proved to be a handicap. Many a student and ex-student took his turn at Englishing Bernd’s prose. He finally pressed me into service, probably because I affected an (unwarranted) air of absolute confidence in this field. Curiously, Willie, whose written English was hypercorrect, never offered to help with any of these rewriting chores.

It didn’t take many years for the Professor

Matthias

A Personal Memoir

to become known, not only on campus, but as far away as downtown San Diego. Even the police would recognize the tall, bespectacled figure with wavy hair, dressed in a black sweater or dark blazer, and driving a slightly disreputable but vintage Oldsmobile convertible. Bernd was certainly familiar to a large body of undergraduates who knew little or nothing of his scientific achievements. This may account for the immediate success of his experimental general education course “Frontiers of Science,” which was meant for, and in fact attracted, a large untutored audience. (To some of us, the course was known by a less flattering name which pointed up its lack of significant content.) In conducting this course, Bernd depended heavily on his friends to provide 45-minute lectures on almost anything. He even managed to persuade four or five of us from Los Alamos to run the gantlet of inattention or restless incomprehension that characterized the audiences of the earlier years. I came to talk about some simple aspects of so-called elementary number theory; “elementary” here has a technical meaning that does not include the notion of simplicity. Bernd liked number theory. On introducing me, he said (inaccurately) that it remained the one subject that was of no use to the military. For my talk a great concession was made: dogs, especially dogs in heat, were barred from the lecture hall. In the course of my short lecture I had occasion to refer to quantities like 10^3 , 3^{27} , and so forth. My wife, Carol, was in the audience and heard one young lady ask her companions “What does he mean by ‘ten-to-the-three’?” Her friends laboriously explained. At the end, Bernd announced the topic for the next meeting. He pointed to an extremely attractive girl in the front row and told us that she would reveal what she had learned about the mystery of the Bermuda Triangle. I’m sorry I missed it.

The Social Animal

Just as there were for Bernd three main centers of low-temperature research, so there were three crowded and demanding social schedules. I saw the Bell Laboratories version only twice, but I got to know something of the La Jolla scene from 1962 to 1980, when Carol and I went out almost every February to spend two or three weeks. During this time I would work for Bernd; the tasks were always different, but they had in common the property of being well within the range of any number of indigenous mathematicians. As it was, Bernd didn’t know any of them, and he would only let his friends do his work. In the 1960s—when we were still young enough to stand the pace—Carol and I would seem to be caught up in a whirlwind. Joan always maintained that things had been much less hectic before we arrived and would settle down again after we left. I must say that neither my wife nor I was ever convinced of this, and I think that Willie and Mossa Zachariasen shared our skepticism. But La Jolla was just two weeks for us; in Los Alamos it was all summer. Here the Matthiases maintained a brutal pace from the early 1960s until Bernd’s death. After Joan’s parents moved to Santa Fe, we would see the Matthiases over Christmas too.

Bernd and Joan needed no help from us or anybody else in making friends either in Los Alamos or Santa Fe. By 1968 they probably had a larger circle of New Mexico friends than we did, and they had spotted us 15 years. If I remember correctly, however, I did introduce Bernd and Joan to Eliot and Aline Porter, a social act that I view as a positive contribution. (I had known Eliot for several years; at the time Bernd met him, I had just taken up large-format photography at his suggestion.) After an uncertain start, Bernd and Eliot became close friends, spending their time in verbal give-and-take whenever they were together. Both of them were unflappable in argument, vehement but

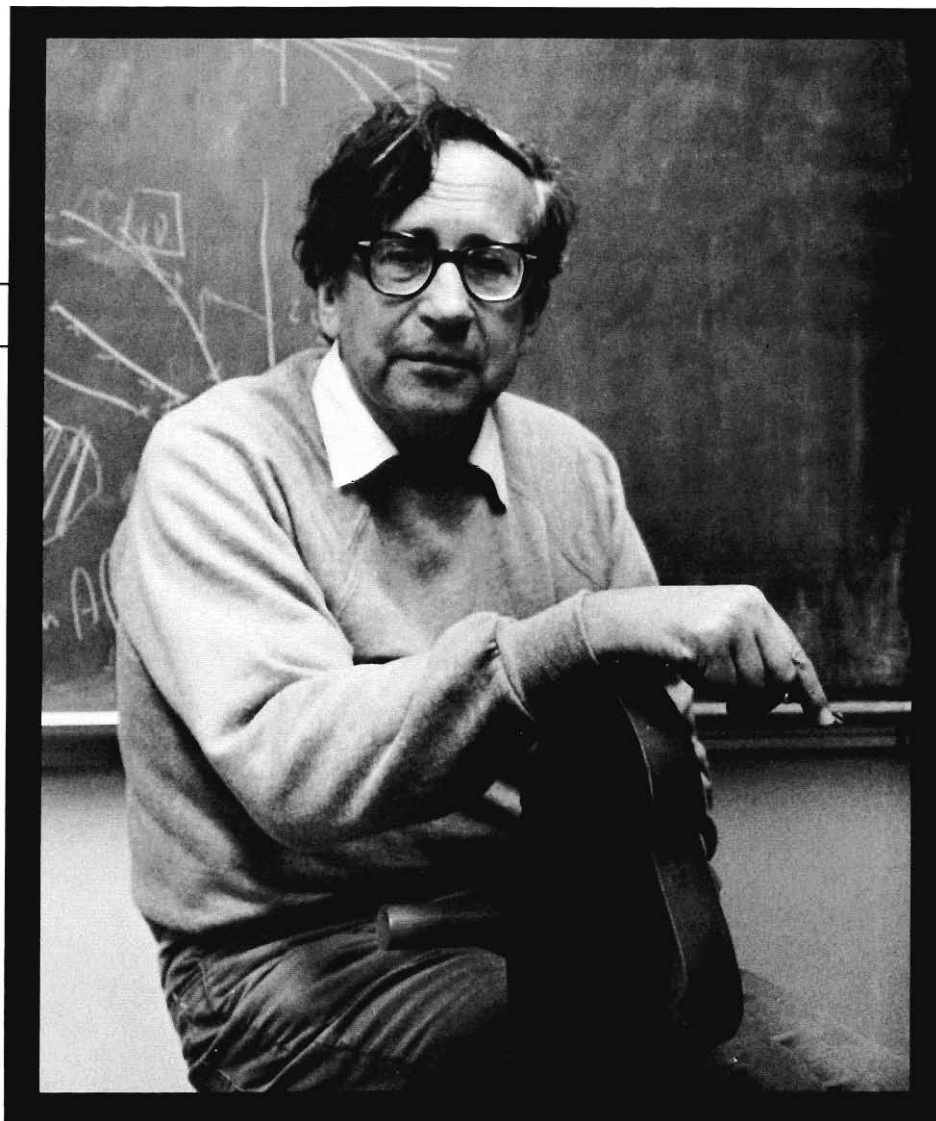
without rancor; it was a very good match.

In their memoir, Fisk, Maple, and Webb say of Bernd: “Like most people who are never bored, he demanded excitement. This meant that much of the time he would produce it himself.” He could do this because in social situations he was totally aware of everything that went on about him. If conversation was in danger of stagnating, or if some awkwardness threatened to intrude, he always knew exactly what to do. Somewhat paradoxically, he was at his best in private conversation—what current jargon calls one-on-one situations. It was not a matter of charm; his interest in others was sincere, and was therefore reciprocated. As I wrote in another place, he was a collector—not of material things (as I am) but of friends. All these friends thought they had a special place in Bernd’s life. I think that all of them were correct.

Iceland

In the summer of 1972 Carol and I went to Iceland as part of a group of eight, the others being Eliot Porter, his son Jonathan and daughter-in-law Zoe, Joan Matthias, and Tad Nichols and his wife, Mary Jane. The males in the party were all photographers with various degrees of professional commitment, with Eliot the acknowledged leader. It was a busy summer for Bernd, but he had promised to join us for a few days at the beginning of our stay. This was the year of the Fischer-Spassky world championship chess match in Reykjavik, so the date is not in doubt, and for once $\Delta t_B = 0$.

We all arrived on the same day—all but Bernd, that is—and, after collecting the duty-free alcohol of our choice, we repaired to the hotel Borg, a small, neat establishment with an excellent dining room. The mood was euphoric, the promise of adventure heightened by exotic details like the signs with impossibly long and unpronounceable names, reminiscent to me of the road signs in Turkey.



In the next few days we were busy with preparations, but there was time to explore the nearby countryside, in particular the desolate, wind-eroded landscape of Krysuvik to the east of the city, where the rocks resemble the wings of fabulous animals. In Reykjavik itself the leaden skies exhilarated me as the bland blue of New Mexico has never done (a photographer's reaction). I wanted to get going. But where was Bernd? Joan was in touch with him; it seemed uncertain whether he would make it. And then he was there, for him reasonably well equipped. At least he'd brought suitable footwear and a selection of sweaters (it was mid June, but the temperature rarely exceeded 45 degrees). Exactly half the group were strangers to him, but that obstacle was swept away in an hour or two. That night in the hotel dining room Bernd absorbed the ambient euphoria and began to talk. After a bit, voices started to rise. Eliot accused Bernd of talking too much, the accusation was returned, and of course there had to be a

bet: the first of the two to say a word would forfeit five dollars. Eliot won by deceit. He left the table, ostensibly to go to his room. Instead, he lingered outside the elevator for a minute or so, then re-entered the dining room and caught Bernd talking. Bernd was outraged by this trickery, but he paid.

Bernd stayed with us for three or four days. We took him to the starkness of Krysuvik and to the fantastic geothermal displays near the city, great roaring jets of steam rushing out of the earth. We took our rented Land Rovers cross-country through the heather. Bernd good-naturedly criticized other people's driving over the non-roads, and when it was his turn at the wheel, got as good as he gave. Each night we tried another restaurant, happily discussing its merits and faults vis à vis those of the previous night's choice. All through this Bernd kept up a running commentary on the state of the union, the coming presidential election, the condition of physics in the United States, the administration of the Laboratory, in short,

anything that had interested him lately. It was as though a much-traveled relative had come home, bursting with tales of distant lands and exotic customs. In reality, we were the ones in the distant land, but it didn't matter.

I think Bernd would have liked to stay longer, but a Senate committee had requested his testimony. So suddenly he was gone. We were not concerned; we knew we would see him soon again, and of course we did. This time it is different, and that hurts. ■

Acknowledgment

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Further details about Bernd and his work may be found in the following publications.

Journal of the Less-Common Metals 62 (November/December 1978). This volume, titled "On the Physics and Chemistry of Solids," is dedicated to Bernd T. Matthias on the occasion of his 60th birthday and contains much information about Bernd and his professional career.

B. T. Matthias and P. R. Stein, "Superconducting Materials," in *Physics of Modern Materials II* (International Atomic Energy Agency, Vienna, 1980), pp. 121-148.

John K. Hulm, J. Eugene Kunzler, and Bernd T. Matthias, "The Road to Superconducting Materials," *Physics Today* 34, 34-43 (January 1981).

Zachary Fisk, M. Brian Maple, George W. Webb, "Some Recollections of Our Years with Professor Bernd T. Matthias," in "Proceedings of the Ternary Superconductor Conference," Lake Geneva, Wisconsin, September 23-26, 1980 (Elsevier/North Holland, New York, 1981).

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